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INVESTIGATION VERSUS PROPAGANDISM

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The case of the ancient pottery-makers of Vero, Florida, seems really quite worthy of becoming a *cause célèbre*. It has already passed through two stages and is now well on in a third. The purpose of this paper is to urge that this third stage be closed at once by common consent and that the inquiry be pushed on into a fourth stage. The first stage embraced the discovery, the preliminary examinations, and the presentation of the case to the scientific public; the second covered some concerted field investigations by interested specialists and the publication of their results in symposia; in the third stage there has developed a tendency to lapse into isolated discussions with a trend toward controversy, as well as a trend toward premature propagandism. What should constitute the fourth stage may best be considered after such a brief review of what has been presented as shall make clear the status of the case at the close of the stages already past.

STAGE I. THE ANNOUNCEMENT AND PRELIMINARY PRESENTATION

The discovery, as is so often the case, was made incidentally in the progress of industrial work. The preliminary inquiries were of the usual type and the initial announcement was made to the appropriate scientific jury through the venerable *American Journal of Science.* The subject was thus given a fitting introduction and a distinctly scientific aspect from the outset.

The presence of vertebrate fossils in deposits of a late age at Vero was learned as early as 1913. Fossil human remains were found in association with these in 1916. The first announcement by Dr. Elias H. Sellards, state geologist of Florida, was made in July, 1916. A supplementary statement in *Science* was made in October, 1916, and an official report in the *Eighth Annual Report of the Florida Geological Survey.* This stage presents an excellent example of the composite work that falls to the lot of state geologists and their associates, in which it is often necessary for a single inquirer or a small group to pass upon very complex phenomena whose ideal treatment calls for the co-operative work of many specialists.

The case as developed in this introductory stage.—Beneath the site of the finds the basal formation is a marine marl (coquina) commonly referred to a late Pleistocene age, though all or essentially all its fossils belong to living species. It spreads under the whole neighborhood, so far as present evidence goes, and forms an excellent basal datum plane. On this basal formation the retiring sea built sand flats and beach ridges, while the wind built dunes, the whole forming a low belt of finishing deposits. These lie along the margin of the present mainland, while across a sea inlet about a mile wide, lies a barrier reef now in process of building which forms the outermost coast line of the Atlantic. The low ridged belt on the edge of the mainland is known in the discussion as Vero Ridge.

After the withdrawal of the sea, surface wash deposits and fresh-water marls gathered to some extent in the shallow basins of these finishing formations. At the same time a small drainage system was established near Vero, now known as Van Valkenburg's Creek. The main stream channel was formed by the junction of two branches which in turn were sub-branched. All these features seem to have been determined mainly by the ridglets, swells, and hollows of the retiring sea deposits. The branchlets, however, reach back into a marshy tract west of the Vero Ridge. The united drainage from the north meets that from the south nearly head-on and the two unite into a trunk stream flowing eastward to the sea about a mile away. The particular site of the human and vertebrate remains lies near the junction of the two branches.

Before the remains under debate could have been buried in the way in which they were found, the creek must have excavated a relatively broad flat with such low gradients as to permit aggradation. The average depth of the aggraded creek deposits is about six feet. They consist chiefly of washed sand and vegetal muck and other débris, with some fresh-water marl. They are thought by Dr. Sellards and most others to be divisible into two layers, a lower, No. 2, consisting of three to four feet of sand and muck, with fresh-water marl; while the upper, No. 3, consists more largely of muck and vegetal débris with a less proportion of sand, layer No. 1 being the marine marl below. The upper layer, No. 3, cuts here and there into No. 2, sometimes cutting entirely through it into the marine bed below.

In this first stage of inquiry, parts of two human skeletons were recognized, one of which was referred by Dr. Sellards to the lower creek deposit, No. 2, and the other to the upper creek deposit, No. 3. It was urged strongly in the initial papers that the scattered condition of the bones, as well as their other relations, shut out the probability, if not the possibility, of intrusive burial. On the other hand, it was urged that the human remains could not have been washed in from outside sources, or even transferred from the one layer to the other in any notable degree, because of the unworn state of some of the fossils and the fragile condition of others. A considerable number of the vertebrates found in the lower layer were identified as extinct forms; some of them had been usually referred to rather early stages of the Pleistocene. A smaller number of extinct vertebrates were found in the base of the upper layer, at about the same level as that of the human skeleton referred to that layer. The bones of the extinct vertebrates and of man were both found to be mineralized in much the same degree. With the human bones were found chipped stones, bone implements, and fragments of pottery.

On the basis of the evidence thus gathered the lower creek deposit, No. 2, was referred confidently ot the Pleistocene; the upper layer was regarded also as probably Pleistocene, but some reservation was expressed respecting this.

STAGE II. CONCERTED INVESTIGATION AND CO-ORDINATE PUBLICATION

Closely following this introductory stage Dr. Sellards extended cordial invitations to various specialists to inspect the deposits and the material already gathered from them, and to join in further inquiry. Two field conferences followed. The earlier and larger was held in October, 1916, participated in by Dr. George G. MacCurdy, anthropologist of Yale University, who gave special attention to the archaeological evidence; Dr. Aleš Hrdlička, anthropologist of the U.S. National Museum, who gave special attention to the human bones; Dr. Thomas W. Vaughan, geologist in charge of the coastal plain investigations of the U.S. Geological Survey, who viewed the matter from the standpoint of the regional geologist; Dr. Oliver P. Hay, research associate of the Carnegie Institution in Vertebrate Paleontology, who studied in particular the evidence of the vertebrate bones; Dr. Rollin T. Chamberlin, associate professor of dynamic geology at the University of Chicago, who gave special attention to the physico-dynamical vestiges of the deposits and their import; Assistant Geologist Herman Gunter, of the Florida Geological Survey; Mr. Isaac M. Weills and Mr. Frank Ayers, citizens of Vero, who had taken an active interest in the discovery from the outset.

The results of this first conference were gathered into a symposium in the January-February, 1917, number of this *Journal*. The contributions were from Dr. Elias H. Sellards, embracing a report by Dr. Robert W. Shufeldt on the fossil birds (pp. 4–24); Dr. Rollin T. Chamberlin (pp. 25–39); Dr. Thomas W. Vaughan (pp. 40–42); Dr. Aleš Hrdlička, including a letter from Professor William H. Holmes (pp. 43–51); Dr. Oliver P. Hay (pp. 52–55); and Dr. George G. MacCurdy (pp. 56–62).

The second conference was held in March, 1917, Dr. Sellards again serving as host, participated in by Professor Edward W. Berry, paleobotanist of Johns Hopkins University, who gave especial attention to the testimony of the fossil plants; Dr. Rollin

T. Chamberlin, of Chicago, who returned for a more critical examination of certain features than was possible on the former visit; Assistant Geologist Herman Gunter, Mr. Isaac M. Weills, and Mr. Frank Ayers.

The results of the second field conference appeared in the October-November number of the same volume of this *Journal*. The contributions to this were from Dr. Elias H. Sellards (pp. 659–60), Dr. Edward W. Berry (pp. 661–66), and Dr. Rollin T. Chamberlin (pp. 667–83.)

The development of the case in connection with the two field conferences and the symposia.—It is aside from the purpose of this discussion to review these papers as personal contributions; we merely wish to trace the evolution of the case by contributions which represent different points of view and different methods of inquiry. We shall therefore merely try to bring out the most vital features of these papers as an additional résumé of the official composite work and as contributions from the standpoints of physico-dynamic inquiry, of vertebrate paleontology, of human osteology, of the human artifacts and industries, of fossil plants, and of other sources of evidence.

Preliminary to such outline, it may be helpful to remark that the Pleistocene age of the ancient men of Vero was pressed to the front as the specially vital question of the case at the outset. This specific question gave trend to the inquiry and to the contributions we are to sketch. Now, a dominating question of this kind inevitably forces the discussion to center, consciously or unconsciously, on the nominal or the conventional aspects of the case implied by the technical term Pleistocene, rather than on the strict realities involved in the case. The crux of the problem is thus made to hang more or less largely on technical terms. These in turn depend on the criteria that are accepted as guides in fixing the use of these terms. This grows naturally out of the history of attempts to show that man existed in America at certain early stages.

Three things are to be kept in mind in interpreting a discussion dominated by such a foremost feature: (1) the term Pleistocene is not used in precisely the same sense by all writers, (2) the criteria used in establishing the technical age are subject to challenge, and

(3) there are more vital questions than the simple Pleistocene or non-Pleistocene age of man in America.

Distinct foreshadowing of these points appeared in the conferences and symposia. The most significant of these was a rather distinct challenge of the trustworthiness of some of the criteria in so far as applicable to the case in hand, though heretofore regarded as good; modifications of the accepted criteria in certain vital aspects were suggested. This of course really means that they are not criteria at all, as used, and will become such only when properly revised. The stress of skepticism was thereby put on the criteria; and so the trustworthiness of these comes to the front as really the most vital feature of the case.

Now the criteria originally used in determining the divisions named Eocene, Miocene, Pliocene, Pleistocene, and Recent were made to rest on the numerical proportions of living species represented in the respective formations, viz.: in the Eocene, less than 5 per cent living species; in the Miocene, 20 to 40 per cent; in the Pliocene, more than half living species; and in the Pleistocene, about 95 per cent.[1] The species counted were chiefly, though not exclusively, marine. As the rates of change of marine forms differ from those of land forms and those of the land forms differ among themselves, this numerical basis was found unsatisfactory from the outset. As the real needs of the case developed, these original criteria were abandoned more or less completely and are now used only in a residual way in certain modified forms and in certain special situations. So far as the Pleistocene is concerned, the numerical paleontological criteria have been replaced to a large extent by physico-dynamic criteria drawn from the great ice invasions. This replacement constitutes a radical and significant departure. It is a part of the general movement to make the causal and world-wide agencies the basis of classification rather than their products, which vary with regional conditions.

For the close of the Pleistocene, in particular, the retreat of the continental glaciers from the plains of Europe and North America and the accompanying climatic effects are now the more generally accepted criteria. The substages of the Pleistocene are

[1] See Dana's *Manual of Geology*, fourth edition (1895), pp. 879–880.

marked off naturally by the successive ice invasions and retreats, because these very profoundly affected all the life in the middle and northern latitudes of the Eastern and Western hemispheres, while indirectly they greatly influenced the life-conditions in the more southerly latitudes.

Though these new physico-dynamic criteria do not afford very sharp lines of demarcation, they are in reality sharper than the paleontological criteria they have replaced. Changes of species are slower, and their interpretation more uncertain, than the changes in the great ice-sheets. It will help to show the real significance of the diverse interpretations of the Vero case if we here give a table of the relative dates of the several glacial stages compiled from estimates made by the more experienced students of glacial phenomena. Of course these are by no means final figures; they are held subject to very considerable changes as additional data are gathered; but they may be regarded as indicating tentatively the order of magnitude of the time-distribution of the glacial stages. We have derived from the compiled estimates what seem to us conservative minimum estimates.

ESTIMATED TIME-STAGES OF THE PLEISTOCENE PERIOD

	Compiled Estimate	Minimum Estimate
Close of Pleistocene, beginning of Recent	25,000 yrs. ago	20,000 yrs. ago
Climax of Late Wisconsin glacial stage..	40,000 " "	30,000 " "
Climax of Early Wisconsin glacial stage.	95,000 " "	60,000 " "
Middle of Peorian interglacial stage.....	135,000 " "	90,000 " "
Climax of Iowan glacial stage..........	180,000 " "	105,000 " "
Middle of Sangamon interglacial stage..	260,000 " "	155,000 " "
Climax of Illinoisan glacial stage.......	340,000 " "	190,000 " "
Middle of Yarmouth interglacial stage..	500,000 " "	275,000 " "
Climax of Kansan glacial stage.........	660,000 " "	330,000 " "
Middle of Aftonian interglacial stage....	900,000 " "	450,000 " "
Climax of Jerseyan glacial stage........	1,200,000 " "	540,000 " "

Maximum estimates should probably be double the compiled estimates in the later stages and more than double in the earlier stages.

Now, if we take the extreme ages to which the fossils at Vero have been referred by different members of the conferences, the total range, using the minimum estimates of our table, is over four thousand centuries, and yet the deposits embrace a thickness of only six feet, the lowest layer of which was not deposited until some time after the last retreat of the sea from the mainland. This serves to indicate the stress the case has put on accepted criteria. With this aspect of the case in mind, let us note the essence of the several contributions to the symposia.

Further contributions from the viewpoint of the state geologist.—The two further contributions of Dr. Elias H. Sellards have the same import as his earlier papers already sketched. He, however, adds certain more declared statements that bear strongly on intrusive burial, on transfer by water and other natural agencies, and on original burial where now found.[1] Among these are the absence of whole skeletons, the infrequency of even complete individual bones, the scattered and imperfectly preserved state of the bones, their frequent and often sharply broken condition, together with evidence that "in the case both of the human and of the other vertebrates the bones were more or less disturbed after they had lost enough of their organic matter to become sufficiently brittle to break as they were moved about by water before reaching their final resting place."[1] "In the number of bones that have been obtained representing a single individual there is observed no important difference between the human and the other animals."[2]

The contribution from the physico-dynamic inquiry.—At the first conference an attempt was made by Dr. Rollin T. Chamberlin[3] to discriminate as closely as practicable what had been done at the successive formative stages, whether constructive or destructive, so as to give an analytical basis for the treatment of the special question under debate and for reconciling, so far as might be, the seemingly incompatible import of different criteria as then interpreted. The several episodes distinguished were as follows: the already well-recognized coquina marl, the datum base (Sellards' No. 1); the sand-capping over this and the formation of beach ridges and dunes; the gathering of fresh-water marl deposits, containing vertebrate remains, in the hollows of this low ridged tract; the formation back of the ridge—more or less contemporaneously with this—of a more extensive flat area with boggy tendencies, the most significant feature of which is an indurated black sand, from which pebbles ranging up to "cannon balls" were later derived and borne into the lower and upper fossil-bearing layers (Sellards' No. 2 and No. 3); the development of the drainage system of Van Valkenburg's Creek, partly in the one and partly in the other of these two upland formations; the excavation of the creek trench to a breadth and gradient suitable for the beginning of aggradation; the first stage of aggradation (Sellards' layer No. 2); and the second stage of aggradation (Sellards' No. 3).

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[1] Symposium 1, *Jour. Geol.*, XXV (January-February, 1917), 1, 22.

[2] *Ibid.*, p. 23.

[3] *Ibid.*, pp. 25-39.

On the basis of this analysis, a hypothesis was proposed which seemed to reconcile some of the apparently conflicting imports of the fossils in the lower and upper parts of the creek deposits. It postulated that the oldest extinct mammals came into the region as soon as it emerged from the sea, and were buried in the hollows and flats of the ridged tract, and more especially in the poorly drained, flattish area lying west of the Vero Ridge, whence at some time notably later they were carried into the creek deposits much as the black pebbles from the indurated sandstone of the same area were found to have been.

No time was available during the first conference after this hypothesis was formulated for testing it, and it was offered in this state in the first symposium as a hypothesis of reconciliation, and the earliest opportunity sought to put the hypothesis to the test. On his second visit, Dr. Chamberlin found the bog deposits back of the ridge essentially barren of fossils, suggesting conditions either inhospitable to life or unfavorable for its preservation. In his contribution to the second symposium the part of the hypothesis relating to the bog deposits was therefore frankly laid on the shelf; nor was the alternative phase which made the upland marl beds of other parts of the tract the source of the older fossils of the creek deposits further urged, because fossil-bearing deposits of this kind were not found in available situations, though they occur elsewhere. The part played by this stage between the retreat of the sea and the aggradation on the bottoms of Van Valkenburg's

Creek must of course yet be worked out and put in its place in the history of the formations at Vero.

On the shelving for the time being of this hypothesis of reconciliation an endeavor was made to detect such physical possibilities for faunal deployment as might be implied by the creek deposits themselves, thin as they are. The inquiry brought out the fact that the exposed sections of the creek filling showed distinctly more reworking in some portions than in others, together with the suggestive fact that where the layers were least irregular a fair line of distinction could be drawn between an upper and a lower portion, but that where there was most evidence of scour-and-fill the line of distinction between upper and lower layers was more obscure and at points quite uncertain. Discussion of the precise line of division between the layers, among the investigators while on the ground, brought out notable differences of opinion, and furthermore opinions were more or less changed as the inquiry went on. As the result of such changes of opinion, the dividing line was more often shifted downward than upward.[1] The effects of scour-and-fill in the more disturbed portions of the deposit were found very notable, and they were more pronounced in and near the critical horizon between the upper and lower deposits than at other levels. Scour holes were traced well down into the horizon assigned to the lower layer (No. 2), and even entirely through it into the marine marl below. It was observed that coquina shells had been carried up across the whole horizon of the lower layer and redeposited in the horizon of the upper layer. A careful excavation of a scour hole led to the successive placing of the dividing line between the layers at lower and lower levels, as the work went on, and in the end to the disclosure at the bottom of the hole of the carapaces of two turtles, apparently put there by the swirling water-hole action.

It was further observed that the portions of the creek deposits most marked by scour-and-fill action were the portions richest in human and extinct vertebrate remains, suggesting some causal connection between the scour-and-fill action and the accumulation of the relics.

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[1] For details and illustrations see Symposium 2, *Jour. Geol.*, XXV (October-November, 1917), 673–82.

It appeared further that, while some of the relics were very fragile and could only be supposed to have been moved by very gentle processes—if moved at all after they reached their present condition—most of such fragile material belonged to the lower layer or to the more undisturbed portions of the upper layer, and so a correlation between the condition of the layers and the condition of their content was suggested.

The question of possible transference was further affected by finding relics of the same species as those to which the fragile specimens belonged in a condition that could easily stand transportation. There remained, however, in some parts of the deposit sufficient fragile material to indicate that it had not undergone more than the most gentle handling since the fragile state was acquired. Such cases, however, were largely in the lower layer and in the less irregular parts of the upper layer.

On the whole, this closer inspection of the deposits and their fossil content seemed to strengthen the presumption that some of the relics of the extinct animals were deposited in the lower layer when that layer was taking on the form it now bears and that these relics were not introduced by any subsequent process.

The import of these observations had the effect of throwing the stress of interpretation on the dividing tract between the two layers and on the upper layer. The termination of deposition of the upper part was definitely fixed at the year 1913, when the digging of the canal stopped the normal work of the creek. It appeared that such bones of extinct vertebrates as were found in this upper layer were much scattered; that their number was small relative to the whole number in the original skeletons, being commonly a few teeth, or a lower jaw, or one or two other bones, which did not seem to exclude some reworking but rather to imply it.[1]

In the light of this inquiry along physico-dynamic lines, the balance of evidence, taken all together, seemed to Dr. Chamberlin to favor the reference of such portions of the creek deposits as bear human relics to the later stage of filling, since all observers were agreed that the upper fill actually penetrates deep into the lower fill at several points, while the more critical examinations aided by excavations increased the evidence of such penetration.

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[1] For specific details on these critical points see Symposium 2, *Jour. Geol.*, XXV (October-November, 1917), 676–82.

Such seeming to be the import of the physical evidence, the testimony of the human bones and of the relics of human industries interpreted on their own grounds seemed to furnish the most available and precise criteria of the paleontological order that was available, and among the human relics the pottery seemed to give the most specific evidence. And so, adding the anthropologic and archaeologic testimony to the physico-dynamic evidence, Dr. Chamberlin concluded that the time of deposition of the upper layer belonged within the Recent Period, perhaps not dating back beyond the mid-Recent stage.

The contribution from vertebrate paleontology.—Dr. Oliver P. Hay identified the vertebrate fauna found in the lower part of the creek deposits at Vero as

> essentially that which is found in the Aftonian interglacial beds of Iowa and in the Equus beds of the Plains. From the latter, this fauna may be followed into Texas, thence eastward into Florida and South Carolina. Of this fauna, two species of elephants, the common mastodon, *Megalonyx*, and the giant beaver continued on until after the Wisconsin glacial stage. Other species, the saber-toothed tigers, *Equus complicatus*, the tapirs, most of the extinct bisons, and *Mylodon* probably disappeared before the Wisconsin glacial stage. In the earlier Pleistocene deposits only are found *Elephas imperator*, camels, several species of horses, and many edentates. At Vero have been found three species of horses, at least four edentates (including *Mylodon*), and a camel. *Chlamytherium* was originally found on Peace Creek in deposits which were then supposed to be Pliocene. In the same deposits was found a jaw containing a tooth of an elephant which is quite likely *E. imperator*. This species has not yet been found in No. 2 at Vero, but about three miles west of the place Sellards found a lower jaw which belongs probably to this species. It is known from Dallas County, Alabama, and from Charleston, South Carolina. [Dr. Hay regards it and camel remains as particularly indicative of the Aftonian fauna.] It is possible that this fauna continued on for another stage or two without great change, but by the time of the Illinoisian drift it had become essentially modified.[1]

Dr. Hay regarded the vertebrate fauna of deposit No. 2 as primary, thus making the deposition of the beds as now seen also Pleistocene in age.

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[1] Symposium 1, *Jour. Geol.*, XXV (1917), 54–55.

> No place was discovered from which the included bones and teeth might have been washed in, nor do they in general have the appearance of transported fossils. These bony remains are in what may be regarded as a normal condition; as when, in a little valley furnishing food and drink and shade, herbivorous and carnivorous species had resorted and perished there for thousands of years. In a normal way their bones have almost all fallen into dust. Some, buried under somewhat favorable conditions, endured longer, but softened and were trampled into fragments by succeeding generations of elephants, mastodons, horses, bisons, huge ground sloths, and smaller forms. Only the most favored and protected bones and teeth have endured to the present, mostly scattered, but sometimes remaining associated with others of the same skeleton.[1]

Dr. Hay regarded the lower part, at least, of No. 3 as also Pleistocene.

> This deposit is somewhat more difficult to work for fossils, but it has furnished almost all the forms that are found in No. 2. It is not improbable that some bones and teeth were redeposited from the lower stratum, but not, I think, any considerable or essential portion of them.[2]

In the *Ninth Annual Report of the Florida Geological Survey*,[3] Dr. Hay treats more fully the vertebrate remains from the beds in question, mostly those of the upper layer, and reaffirms his belief not only that the fossils represent an early or middle Pleistocene fauna, but that the deposits in which they are now embedded are early and middle Pleistocene in age also.[4]

Dr. Robert W. Shufeldt described the fossil bird bones but does not discuss their bearing on the age of the deposits further than to indicate that some of the species are extinct.[5]

Contribution from the standpoint of paleobotany.—In his report on the fossil plants, Dr. Edward W. Berry says that "plant remains in the form of laminae of impure peat or scattered fruits, chiefly acorns, are present from the bottom to the top of the deposits overlying the shell marl which forms the base of the section. The lower sands (designated No. 2 by Sellards) have yielded no leaves and but few acorns, but the upper bed (designated No. 3 by Sellards) contains many leaf layers alternating with sand laminae."[1]

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[1] Symposium 1, *Jour. Geol.*, XXV (1917), 52.

[2] *Ibid.*

[3] Pp. 43–68.

[4] *Ninth Annual Rept., Florida Geol. Survey*, p. 67.

[5] Symposium 1, *Jour. Geol.*, XXV (1917), 18–19; *Ninth Annual Rept., Florida Geol. Survey*, 1917, pp. 35–42.

> Recent and extinct mammalian and other bones occur in both layers, and human remains are also found in both beds. After a thorough study of the local sections and the paleontologic evidence I am convinced that there is no hiatus between beds Nos. 2 and 3 and that there is no great difference in age from the bottom to the top of the section, although it records changing physical conditions and necessarily becomes gradually more and more recent as the top of the section is approached. The lower sand marks the recession of the sea in which the underlying shell marl was formed. The upper beds (No. 3) mark successive seasonal layers of valley filling in the narrow valley of a small stream. This stream was apparently always small, and the marvelous abundance of fossils at this one point seems to be due to a bar or sink-hole or similar cache formed near the junction of the two lateral branches which united near this point to form the main stream. The determinable plants are represented almost exclusively by fruits or seeds, as the leaves, with the exception of the coriaceous oaks, which are abundant, were too thoroughly decayed before they were buried to retain their identity.[2]

After a summary grouping and discussion of species, Dr. Berry concludes:

> In my judgment and in the ordinary acceptance of that term this flora is unquestionably of late Pleistocene age.
>
> Regarding its bearing on the interesting problem of the age of the human and associated mammalian and other remains at Vero, my study of the locality furnishes the following somewhat categorical conclusions. The underlying shell marl which forms a definite and undisputed datum plane, is late Pleistocene in age. Its species all exist in near-by waters at the present time. . . . . It follows that the vertebrate remains which are so numerous at Vero cannot possibly be of Middle or Early Pleistocene age unless they are regarded as having been reworked from older deposits, and I cannot conceive that this was possible, nor do the vertebrate paleontologists who have examined the deposits consider that such was the case. . . . . Nothing is more reasonable than to suppose that the larger elements in the Middle Pleistocene fauna of more northern areas should have lingered for thousands of years in this more genial southern clime until the presence of man in considerable numbers and the changing climate, as is attested by the fossil plants, should have brought about the extinction of a large percentage of the fauna. The fauna itself confirms the rather limited data furnished by the fossil flora of this change in climate,

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[1] Symposium 2, *Jour. Geol.*, XXV (October-November 1917), 661.

[2] *Ninth Annual Rept., Florida Geol. Survey*, 1917, pp. 19–20.

since it indicates a more mesophytic habitat than exists today in the vicinity of Vero. . . . . I therefore see no reason to doubt that relatively modern men were contemporaneous with this partially extinct fauna of Middle Pleistocene aspect which survived in Florida to the late Pleistocene. With regard to the exact age of the Vero deposits there are, it seems to me, but two alternatives, and these apply equally and are in large part derived from a study of the physiography and the faunas and floras of the corresponding topographic forms in the other states of the Coastal Plain. These alternatives are that they are about the same age as the Peorian interglacial deposits of the Mississippi Valley or are immediately post-Wisconsin and correspond with what the Scandinavian geologists have named Litorina time.[1]

Contribution from the viewpoint of the fossil beetles.—Recently a contribution relative to the fossil beetles has been made by H. F. Wickham, which is given a place here. The following is the author's summary:

> Two conclusions seem to be warranted after a study of the beetle fragments. The first is that there is nothing to indicate any particular difference in climatic conditions in Florida then and now, since the assemblage of genera is the same as one might expect to find in a stream valley there today. The nearest relatives of the species are still characteristic members of the Floridian fauna and many of them are apparently identical. Second, it seems evident that there has been some change in minor characters of sculpture, since it is not possible to match certain of the fossils exactly with modern forms. In view of the fact that other researches indicate that insect evolution has been extremely slow, so that many species, even as old as the Tertiaries, are rather difficult to discriminate from their modern allies, no more marked divergence would be anticipated.[2]

Contribution from the viewpoint of regional geology.—Obviously the formations that occupy the adjacent coastal region and reach back to the glacial deposits have a vital bearing on the interpretation of the Vero deposits, and so the problem of interpretation as seen from the standpoint of the geologist in charge of the coastal plain investigations of the U.S. Geological Survey, Dr. Thomas W. Vaughan, constitutes a contribution on a distinct basis. The following is the essence of his conclusions: "The occurrence of artifacts and human bones in association with Pleistocene fossils does not prove the Pleistocene age of man," since previous investigations have shown that human artifacts and bones, by many agencies, natural and artificial, have been carried below the surface and become imbedded in unconsolidated deposits. "The only indisputable geologic proof of the Pleistocene age of man must consist in finding a continuous undisturbed bed or layer of demonstrable Pleistocene age above the human remains."[1] In view of the questions that are still undetermined, he advises suspension of judgment for the present.

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[1] *Ninth Annual Rept., Florida Geol. Survey,* 1917, pp. 31–33.

[2] *Amer. Jour. Science,* May, 1919, p. 355.

Contribution from the testimony of the human bones.—Dr. Aleš Hrdlička, after describing critically the human bones, expresses the unreserved opinion that they do not differ in any essential respect from those of the modern Florida Indians.[2] He argues in favor of the human burial of the remains, but this, of course, is geological rather than anthropological and is only incidental to his testimony as an expert student of the human skeletons themselves. He reaches the conclusion that the human bones found at Vero may well be prehistoric, and date from the early part of the occupation of the Florida peninsula by the Indians, but that no proof is furnished by the circumstances of the find, or by the human bones themselves, which would relegate the latter to an antiquity comparable with that of the extinct fossil remains with which they are associated.

Contribution from the viewpoint of the human artifacts.—In a note inclosed in Dr. Hrdlička's paper, Dr. William H. Holmes reports on twenty pieces of pottery collected near the human bones, that

> they represent moderately small, undecorated vessels, apparently simple bowls such as were in common use among the Indian tribes of Florida. Compared with corresponding plain vessel fragments from Florida sand mounds and from occupied sites generally, no significant distinctions can be made; in material, thickness of walls, finish of rim, surface finish, color, state of preservation, and size and shape of vessels represented, all are identical. There thus appears not the least ground in the evidence of the specimens themselves for the assumption that the Vero pottery pertains to any other than Columbian and immediately pre-Columbian time.[3]

The more comprehensive contribution from the archaeological point of view was made by Dr. George G. MacCurdy, who took part in the first conference and symposium. His discussion covers the import of the flint spalls and implements, the bone implements and the pottery. In the course of critical descriptions and observations on these he notes that two of the spalls are identical in material and might well have been chipped from the same flint block. One was found in bed No. 2 and the other in bed No. 3, which he regards as significant and raises the question whether both might not have come from the upper bed through the agency of growing roots or burrowing animals. The spalls were never retouched and were evidently chipped from the parent block near where they were found. As the parent rock is not found in place nearer than the Ocala formation, one hundred miles distant, he thinks the transportation was by human agency. A typical arrowhead of flint with barbs and stem implies the end sought in the chipping and the state of the industry.

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[1] Symposium 1, *Jour. Geol.*, XXV (1917), 41.

[2] *Ibid.*, p. 50.

[3] *Ibid.*, 51.

To summarize the archaeological evidences of man's antiquity at Vero, one can say that the pottery, bone implements, including fishhooks, boneheads, and flint arrowheads from stratum No. 3 and from the surface of contact between it and the stratum below, all point to a period that might well have continued down to the close of the prehistoric period in Florida. This is also true of the human skeletal remains from the third stratum. On the other hand, of the 25 mammalian species from the second stratum as listed by Dr. Sellards, ten, including *Elephas columbi*, *Mammut americanum*, *Equus leidyi* (?) and *Tapirus haysii* (?), recur in stratum No. 3. Assuming that the stratigraphy is not misleading, the conclusion is either that this particular phase of the Neolithic period in America dates back farther than many had supposed, or else that certain fossil mammals continued to live on in Florida until a comparatively recent date.

The chief interest centers in the second stratum. From it no undoubted stone implements have thus far been reported. Although probably produced through human agency, the flint spalls from this deposit do not differ from those in the deposit above, in one case there being absolute identity of material. While a greater number of bone objects have been found in the third deposit than in the second, bone points of the same type occur in both; neither do these seem to differ as to their chemical state. Potsherds, fairly frequent in stratum No. 3, have not yet been reported from the stratum below. Of the human skeletal remains there does not seem to be any appreciable differentiation between those from the second and those from the third stratum.

There are to be noted then the absence of well-defined stone artifacts and of pottery from the second deposit; the presence of both in the third; the similarity

> of the flint chips from the two deposits; the similarity of the bone points in both deposits; and the greater number and variety of bone artifacts, including ornaments in the third deposit. But for the similarity of the flint chips and the bone points, the cultural evidence is very much as one might have been led to expect, assuming of course that the stratigraphy is unmixed and that all specimens have been found *in situ.* On the other hand, in the absence of stratigraphy as a guide, of all the human and cultural remains reported from stratum No. 2, none would seem out of place in stratum No. 3.
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> . . . . The presence of plant stems, acorn cups, and pieces of wood in the second stratum, although by no means so abundant as in the third stratum, nevertheless give to it an aspect of comparative newness. Some of the leaves in the muck at the base of the third stratum look as if they might have been buried only a few years ago.
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> From observations made on the spot and from a study of specimens submitted, the writer is of the opinion that for the most part the human skeletal remains, flint chips, and artifacts probably found their way to this meeting-place of waters through the same agencies as did the various animal and plant remains, and that there has been more or less dove-tailing of the two deposits, because of the peculiar location of the site at the junction of two streams coming from opposite directions. If these premises be true, it would be hazardous to attribute any great antiquity to even the oldest human and cultural remains from Vero. It would be more logical to assume that some of the extinct forms found in the second stratum are perhaps derived from an older deposit; that others lived on in that southern clime longer than has hitherto been supposed, and that the presence of the Indian hunter had much to do with the final ringing down of the curtain on the drama of their ultimate extinction.[1]

It will be seen from these summaries that no consensus of opinion was reached or even approached by the several special students who attacked the problem from their own special points of view. The problem, however, was opened up broadly and the lines of profitable future work fairly well indicated, though not specifically stated. The most radical feature disclosed is the very different degrees of confidence reposed in different criteria. As we have already remarked, the really vital question raised by the Vero case is the value of the various criteria that have been in use and continue to be used.

Whatever else may be said, the outcome offered no warrant for the public propagation of any decision or consensus, pro or con, respecting the Pleistocene age of the ancient Vero man.

[1] Symposium 1, *Jour. Geol.*, XXV (1917), 60–62.

The several views relative to the age of the Vero man may be summarized as follows:

From the viewpoint of the State Geologist—Pleistocene.
From the viewpoint of regional geology—opinion withheld.
From the viewpoint of physico-dynamic geology—Mid-Recent.
From the viewpoint of archaeology—Recent.
From the viewpoint of anthropology—Recent.
From the viewpoint of vertebrate paleontology—Early or Middle Pleistocene.
From the viewpoint of paleobotany—Late Pleistocene.
From the viewpoint of invertebrate paleontology—?

STAGE III. A TREND TOWARD ANTAGONISM AND PROPAGANDISM

The preceding co-operative stage was wholesome and fruitful in a gratifying degree, however indecisive. It is less gratifying to note that some signs of a lapse into polemics have recently manifested themselves. While the utmost independence and the highest critical spirit are eminently wholesome, criticisms from personal points of view that take such a form as to place an opposing view at a disadvantage, either from inadequacy or inaccuracy of statement, do not make for wholesome progress. They are not likely to be altogether fair—however well intended—either to readers or to those criticized. If something in that line is regarded as necessary, it is much better given the form of a joint discussion, preferably with exchange of manuscript beforehand. Such a discussion puts both sides before the reader at the same time and on equal footing. If it is felt that an attack must be made, let it be arranged that the defense go with the attack in the spirit of ancient chivalrous combats and in the not less chivalrous spirit of the most approved form of modern polemics.

Partly in association with more or less polemical reviews and partly independent of them, there has appeared also some tendency to propagate preferred views as though they were determinate while yet about half those who have taken part in the inquiry dissent from such views. Unless the fact of such dissent is duly impressed on the reader, he is placed under the disadvantage of partisan influence, and if he is keenly alive to his rights, he is likely to resent this. The varied opinions cited above seem quite clearly to imply that the time for propagating the Pleistocene or the non-Pleistocene age of man in Florida has not yet come.

But let us pass lightly by this diversive stage and consider whether a rejuvenated effort of the co-operative order may not be made the most worthy successor of the already fruitful second stage.

STAGE IV. FURTHER EFFORTS AT RECONCILIATION AND REVISION

What is the most laudable line of future effort in the light of the disclosures of the preceding conferences? Is it not obviously a concerted effort to evaluate the criteria brought into service, to revise them if found necessary, and to find in an analysis of the case the means of reconciliation of the divergent views? Certainly the true end of endeavor is a concurrent interpretation. To this end should not each advocate stand ready to reshape his view so that it shall be reconcilable, so far as possible, with the evidences that seem to favor some opposing view? We may be sure that Dame Nature has proceeded rationally in the operations at Vero, as everywhere else, and that there really is complete harmony and consistency in the evidences presented.

We beg to make a few observations by way of suggesting further studies in the interest of such revision and reconciliation, but let it be clearly understood that these are but suggestions to deploy the case for additional investigation; they are not proposed solutions.

a) *Suggestions relative to the creek deposits themselves.*—Present processes are fairly clear and quite demonstrable. Recent processes should naturally be next most clear and most demonstrable. May it not be well then to change the order of inquiry by starting where the evidence is best and try to determine first what amount of aggradational work may reasonably be assigned Van Valkenburg's Creek during the Recent Period? The probable minimum length of this, according to the tentative scale given on a previous page, is two hundred centuries. By analyzing its formative processes, it should be possible to reach some approximate notion of the nature of the recent work of Van Valkenburg's Creek and perhaps of its time-relations. The results projected backward from 1913—when the digging of the canal interrupted the normal work of the creek—for two hundred centuries should give a fairly firm first step in interpreting the actual six feet of creek deposits, if these are to be interpreted as simple aggradational accretions that have escaped reworking. If, on the other hand, reworking has seriously influenced the case, some of the criteria thus far much insisted upon may be brought into question.

Such an inquiry might start from two questions: (1) What is the *normal* thickness of vegetal muck and sand that might reasonably be expected to accumulate in such a situation during the Recent Period? (2) What is the *minimum* amount that can reasonably be thought to satisfy the demands of the case? The first question looks simply to the most rational estimate of work done; the second is influenced by a desire to leave as much as possible of the six feet of the actual deposit for assignment to earlier stages to help reconcile the shallow depth of the deposit with the faunal evidences that imply a great lapse of time. This finds its justification in the fact that a heavy stress is put on interpretation somewhere, and it is in the line of reconciliation for each advocate of a particular view to leave as much leeway as he can consistently for the accommodation of conflicting views. This in the end may be found to be the only way to save his own criteria from serious challenge.

To realize the grave nature of the stress which the case presents in only one of several particulars, let us for the moment suppose that the rate of aggradation on the flood-plain of the creek is no more than one inch per century; the total aggradation of the flood-plain in the two hundred centuries assigned the Recent Period would be more than twice the whole six feet actually shown in the creek sections. The supposition is of course merely illustrative.

Preliminary to any true estimate of an actual deposit of this type, it is necessary to inquire how a creek does its aggrading work on a low gradient plain of its own formation when controlled by a sea-level near at hand. Whatever else is involved—and there is much that cannot be noted here—two quite different classes of work are involved: (1) that done upon the flood-plain which occupies all the creek bottoms not at the time included in the creek channel; (2) that done in the more limited tract of the stream channel itself. It is essential to note the normal differences between these two forms of deposit and their working relations to one another.

(1) The flood-plain deposits are formed of the residue of whatever grows upon the flood-plain, or may fall upon it in any way, or may be blown upon it by the winds, or may be mired in it when wet and soft, together with whatever may be washed upon it by the stream at the times of freshet or flood. When first spread upon the bottom, the deposits of freshet and flood stages are usually stratified, but they are quite sure to suffer serious disruption by the growth of plants, the tread of animals, and various other surface agencies, so that they very commonly lose their original structure. On bottoms of low gradient in a sandy tract flood-plain deposits are thus likely to be a heterogeneous mixture of vegetal and earthy muck combined with more or less sand, and to embrace as incidentals the hard parts of whatever lives and leaves its relics on the flat surface.

(2) The channel deposits, on the other hand, are usually formed mainly of the normal products carried by the stream along its bottom, notably sand and pebbles, but these are quite sure to be more or less mixed with derivatives from the flood-plain deposits that form the banks of the stream. More or less constantly these muck banks are being undercut and caused to slide into the channel where they are then more or less reworked by the stream. Usually the muck masses are gradually disintegrated and washed onward by the stream, but they may be buried bodily in its sands, thus carrying into these sands whatever the muck masses happen to contain. This content is likely to retain whatever condition it happened to have when in the muck bank.

In addition to these normal processes the stream, at appropriate points here and there, is also engaged in the work of scour-and-fill. This accidents the results and adds its special features to the products of the more uniform channel work.

Now this tripartite work is pre-eminently a mixing process; at the same time it is an assorting process. Each of these two processes is likely to be carried to varying degrees in the multitude of individual cases involved; the mixed product thus commonly shows a wide range of variation. In part the mixing may be very gentle and the hard parts in the muck may show very slight abrasion. The slide of the muck banks into the bed of the creek is often one of the gentlest of moving processes, for the muck lubricates and cushions the sliding mass. The matter enveloped in the muck, however weak it may be, is easily slipped along in its cushion without appreciable strain or abrasion. Muck balls may even be formed and rolled notable distances, carrying in their protecting embrace structures of any degree of fragility until burial shall finally fix them in the stream sands or disintegration shall free them for a cleaner burial in the same sands. The typical channel material in the stricter sense, that is, the material which has been rolled along the stream bed for considerable distances, perhaps from sources beyond the aggradational tract, is normally much more worn, whether it be rock or bones. The several parts of the creek deposit are thus likely to show the effects of very different experiences. Only a critical study of each individual relic is usually sufficient to disclose its special history. Normally there is a very wide variation of conditions ranging from relics that remain almost intact to those which are very much abraded.

Now it is the normal habit of a stream that meanders on a plain of its own formation to shift its channel gradually downstream by undercutting or eroding its banks preponderantly on the down-stream side. In the course of time the stream thus slowly creeps over its whole flood-plain, and in so doing cuts away and reworks a large part of the flood-plain deposit. As the meanders thus slowly shear their way over new ground, new flood-plain deposits are formed on the ground abandoned by them; thus the flood-plain is rebuilt behind the shearing meander and made ready to be cut away again when the next meander reaches it in its progress down-stream.

Now, as already noted, this shearing process is attended almost inevitably by the special supplementary work of scour-and-fill which bores its holes below the common channel-level at the points of specially forceful or turbulent action. This is particularly characteristic of streams subject to great variations of vigor, as for example streams in regions of alternating dry and wet seasons, as in the Vero case, especially when heavy rains are frequent and even typhoons are occasional. This scour-and-fill action is a special mixing process superposed on the more general ones already sketched.

Now if, in the case in hand, only a foot or so be allowed for the flood-plain deposits in process of growth, and only a foot or so for the normal channel-work, and only a foot or so below that for the deeper scour holes here and there, the vertical range of this active formative and reworking process is too great to be neglected when there are only six feet of deposits altogether to be dealt with. It must be kept in mind that this working combination, now at the top, has in the process of forming the six-foot deposit gradually worked up from the bottom, leaving some portion of its base at successive stages to help build up the lower part of the six feet. This lower part is thus an accumulation of residues from the working combination.

It is therefore necessary to distinguish between the working group at the top—which in the case in hand ceased its formative process in 1913—and the succession of residual deposits below it, these being merely the truncated bases of the formative deposits which the successive meander-shears have left behind. The group in process of formation at the top belongs not so much to the Recent Period as to the current working stage. Setting this aside as merely the record of the work in progress when suspended, how much more is to be added from the rest of the six feet below to make up a reasonable record of the past two hundred centuries, more or less, which, according to our tentative scale, constitute the Recent Period? Will anything less than the two or three feet of the upper layer, No. 3, suffice? Indeed, is that enough?

This method of proceeding from the known to the unknown seems eminently sound and helpful up to this point; here it encounters difficulties and these show its limitations. Theoretically, the several basal accretions left by the successive meander-shears may (1) have some notable thickness, (2) or very little thickness, or (3) even a minus thickness, i.e., there may be loss of previous gains by the shearing away of more than has been built up since the last shear. Besides this, the time between meander-shears is also more or less uncertain. Just what the actual amount of accretion was in each intershear interval, in the case in hand, can only be determined by an inspection of the accretion layers themselves, if indeed their condition permits this. Even that may not suffice for a positive determination, but it is worthy of trial. Herein lies one of the calls for further investigation before anything purporting to be a final conclusion is put forth.

The method of inquiry here suggested, in spite of its limitations, clearly has these good points: (1) It begins with what is best known and forces a recognition of the formative group of deposits at the surface; (2) it suggests a specific kind of examination of the beds below this working group for specific data; (3) it invites a very critical study of the effects of transfer from one formative deposit to another under conditions whose possibilities are both great and highly varied; (4) it is well suited to throw light on the significant fact that, while only a very small fraction of the total number of bones belonging to any individual skeleton have been found in the deposits in question, implying that the great majority have somehow been carried away or otherwise disposed of, some of these are yet not necessarily much worn; (5) it is well suited to explain why much-worn material and little-worn material occur together; and (6) it makes altogether natural a gradation from the older to the younger content, attended with intermixture.

If the normal mode of formation of creek deposits were the only consideration to be weighed in the matter, the six feet of the deposit might perhaps be so handled in interpretation as to plausibly represent the stream aggradation during all the time between the Aftonian interglacial epoch and the present, though that would seem to be putting a very heavy strain on the probabilities; at least the deposits might not yield any demonstrative evidence to the contrary. So far as it goes, therefore, the method gives a possible basis of reconciliation between the import of the fossils in the basal part of the six feet, interpreted as very old relatively, and the obvious recency of the upper part. The method is certainly helpful in explaining the crowding into close positional association faunas heretofore regarded as very different in age. To that extent it gives relief to one of the most declared phases of stress that make the case a really notable one.

The one feature in this mode of analysis and interpretation that may not be acceptable to some of the interested students of the case lies in the fact that it not only permits but postulates much transfer from a state of first burial in the flood-plain muck to a state of later burial in the channel sands, and to still further rehandling by the scour-and-fill process. However, this is not that phase of transfer that has been most under discussion in the history of the case. The interpretation suggested does not necessarily involve any transfer from an independent deposit somewhere else to the creek deposits. It is purely negative on this question, which is left to be decided pro or con by its own class of evidence.

A possible episode in the history of the Van Valkenburg's Creek deposits must be suggested briefly to meet one other phase of the deposits. There are specific grounds for thinking that the gradient of the creek may have been steepened, for a time, at some period between that of its early adjustment to the sea as it retired and its adjustment that now obtains at about the same level. Some of the streams, harbors, and inlets in the region seem to occupy drowned valleys, and this has been held by Shaler, Matson, and others to imply that a lower relative level of the sea, to the extent of twenty or thirty feet, obtained for a time. Such relative lowering of the sea-level, if it occurred, would naturally have caused Van Valkenburg's Creek to sink its channel—attended perhaps by a flood-plain of greater or less extent also—into the previous aggradation plain. As the sea-level again arose to its present position, the sunken trench should have been refilled until aggradation would be resumed on the level of the old plain. Such an intervening stage in the drainage seems to harmonize well with the browner color of the lower layer and with the division recognized between the lower and the upper layers, the latter of course being the assigned work of the resumed aggregation on the surface of the old plain.

To fill out a fair recognition of the inevitable mixing processes to which the creek deposits were subject in the course of their accumulation, several incidental activities should be noted, the most important of which perhaps is the overturn of the trees that grew on the bottoms; but this paper would grow tedious if these and other incidental modes of mixing were discussed in detail.

b) *Suggestions stimulated by the outer relations of the creek deposits.*—Whatever the above suggestions might contribute toward the interpretation of the crowded and mixed association of the fossils and relics in the six feet of creek deposits, if put into service, they would still leave no little apparent conflict of evidence connected with the neighboring and also with the more remote relations of the deposit.

Geologists studying these outer relations, either from the physico-dynamic point of view or from the regional point of view, are confronted with difficulties that naturally cause them to hesitate to believe that the time since the Vero Ridge was formed—which is somewhat greater than the time since the creek deposits began to be formed—can be the record of all that was done here since the Early or Middle Pleistocene. Inevitably then the presence in the beds of extinct mammals that have been regarded as evidence of these early stages raises a serious question as to which class of criteria is the more trustworthy.

Anthropologists naturally hesitate to believe that the Indian type of skeleton could have remained without appreciable modification for forty-five hundred centuries—the minimum date assigned the Aftonian stage in our tentative time-scale—or for any notable part of that long period, and so on these grounds also the question of the relative value of criteria is raised. In this case, interestingly enough, the conflict of evidence takes the form of bone against bone.

The archaeologists are naturally still more hesitant to believe that Indian pottery-making could remain in a static state for the period named or any period measured by hundreds of centuries, not to say thousands of centuries, for when a plastic art is set over against the evolution of biotic species, the presumption of the speedier change lies much in favor of the art.

The paleobotanical evidence has been interpreted as implying a much shorter period than do the mammalian remains between the lower and the upper deposits of the creek series. The general presumption of changeableness, however, has commonly been held to favor the mammals.

Marine paleontology seems to be rather neutral in this war of criteria. The marine layer that underlies the beach ridge in which the creek sank its valley and later began to fill it contains few fossils, if any, of species other than those now living. Using the inherited organic criteria, the age of this layer has usually been placed at or near the end of the Pleistocene Period. It would seem to be a fair accommodation to the general tenor of the various evidences to make the last retreat of the sea from the mainland in the Vero region the beginning of the Recent Period. Such a reference, however, places the *beginning* of the creek deposits *within* the Recent Period. This conforms very well to the apparent requirements of regional geology, of physical geology, of anthropology, and of archaeology. Perhaps paleobotany might be accommodated to it without putting its data under much strain if any at all; but vertebrate paleontology, especially mammalian paleontology, as heretofore understood, would apparently need radical revision. The suggestion here implied, that the extinct mammals may have lived on even into the Recent Period, goes a step farther than the suggestion made at the conferences to the effect that the several vertebrates heretofore only known to occur in Early or Middle Pleistocene beds really lived on in this southern clime until a much later Pleistocene date. Even then it is not necessary to suppose that the older of these extinct mammals were contemporaneous with man, for the two hundred centuries assigned to the Recent Period give room for appreciable differentiation. Obviously this basis of reconciliation throws the burden of revision most heavily on vertebrate paleontology and subordinately on paleobotany.

Those who are thus hypothetically made to bear nearly the whole burden of revision are entitled to trial hypotheses which shall in a like hypothetical way throw the burden of revision on other criteria. The method of multiple working hypotheses, in its eminent fairness, requires us to make an earnest effort to bring under trial all tenable alternatives, to the end that among these there may be found the one that apportions the task of revision in strict accord with the inherent value of the combined evidence. This in its very nature also covers the question whether or not it is possible to escape all serious revision of criteria by finding some tenable basis of reconciliation not perhaps now recognized.

How such a series of multiple working hypotheses is to be built up from the one we have just suggested is almost obvious, for the one just offered is a rather extreme one and quite suitable to form the end hypothesis nearest us in time. To build out the series from this backward in time, it is merely necessary to push back step by step the assigned place in time of the basal creek deposit and note the degree of strain put on each of the several criteria of interpretation by each step. The measure of total strain in each case—or the best balance of such strain as is unavoidable—forms the index of success of each trial hypothesis.

In studying such a series it will be not only interesting but significant to note that, if we accept the interpretation of the creek deposits already sketched, the extinct vertebrates may be assigned earlier and earlier ages without necessarily bringing any strain at all to bear upon the archaeological and anthropological evidences, for the human relics were buried at horizons so close under the deposits in process of formation when suspended in 1913 that any tenable amount of backward assignment of the content of the lower layers is not likely to carry these relics back beyond the place their own criteria would assign them on independent grounds. The strains that are really brought out by such backward steps in assignments of the date of deposition of the extinct vertebrates or of the plants bear almost solely on the criteria derived from physico-dynamic and regional considerations. The question of the Pleistocene or the non-Pleistocene age of man is thus replaced by the much broader and deeper question, What are the relative worths of the criteria offered by life-extinctions on the one hand, and those offered by physical state, dynamic work done, regional distribution and stratigraphic arrangement, on the other?

In any attempt to rectify the criteria, there is a prerequisite that is strictly indispensable, to wit, the keen recognition that relics buried beneath secondary or tertiary derivatives from the surficial deposits are little better than Mephistophelian in their insidious power to deceive. A relic reported as found beneath twelve feet of gravel or twenty feet of loess by anyone not trained to avoid these Mephistophelian lures is to be held as quite without value until tested; it merely serves as an interrogation point to punctuate the question, Is it worth while to investigate the case?

c) *The deeper and broader issues involved in the Vero case.*—The more profound interest of studious men of science lies in such advances in modes of inquiry, such increases in the trustworthiness of criteria, such larger hospitality to working alternatives, such greater readiness to make trial of these and frankly lay them aside if found wanting; such hospitality to revision, and such larger views of the whole problem as may be drawn out from the case. Let us therefore survey from a more general point of view the main lines along which this problem may be approached with the hope of important light. Foremost among these are: (1) the extinction of biotic species; (2) the introduction of biotic species; (3) the physico-dynamic vestiges; and (4) the regional relations of the formations; the rest must be omitted. The leading criterion of the first is *life gone out;* of the second, *life come in;* of the third, *work done;* and of the fourth, *the deployment of the record.*

(1) In approaching a problem on the first line, it has been customary to bring to bear the times of extinction of the species of faunas and floras that lived in the stages immediately preceding and the ratios of such extinct forms to the living species present. Some advances on the older methods are to be noted. Chief among these is the appeal to causes of extinction as confirmatory evidences. This relates the statistical method to the dynamic method; it helps to co-ordinate the facts of extinction with the work of the agencies that produce extinction. This has chiefly been a co-ordination of faunal changes and glacial climates. Further deployment in this and other directions is much to be desired. As a suggestion in that line, why should not the

northward advance of pathogenic microbes, assignable to the rise of temperature as an interglacial epoch comes on, be quite as plausible a cause of extinction as the southward advance of a cold climate as a glacial epoch comes on? In this particular case may not the advance of the warmth of the early Recent Period have thus been indirectly one of the effective causes of that rather remarkable rate of extinction which seems to be recorded in the six feet of the creek deposits at Vero?

(2) The second line of approach has not been sharply distinguished from the first, nor has it been brought into equal use as a means of discrimination, as the discussion of the Vero case very pointedly shows. Has not the introduction of species, however, resources of discrimination equal to those of extinction, if not indeed greater? In the use of extinctions the inquirer can only start with the imperfect knowledge of earlier faunas and floras. He can only trace these forward to disappearance by means of the inferior record of those earlier times. On the other hand, in the use of the introductions of species, it is possible, by reversing the time-order, to start with the fuller knowledge of the present faunas and floras and trace these backward by the use of the more complete record of recent times. There may thus be brought into use a large amount of data relative to the climatic adaptations of existing species, varieties, and races, and hence by inference, knowledge of the climatic conditions that obtained when these were introduced. By starting with the richer resources of the biologic knowledge of the present and the fuller knowledge of present conditions, there is good reason to hope that before the preceding records grow too obscure, the climatic effects of the last ice invasion even upon southern faunas and floras may be detected, and so the southern fossil records rendered directly comparable to the glacial record.

Now it is highly pertinent to the question at issue to observe that the extinctions and introductions of biotic species, varieties, and races during the last century have been phenomenal in kind and degree. Within the memory of some of us that are older there have been profound changes in the general aspect of the regional faunas and the floras in North America more radical, perhaps, than were witnessed by the preceding ten generations. That there was a special biologic cause for this only intensifies its importance as a subject of special study. It is such critical events in history that should put us on our guard, for similar critical events probably characterized all the past, as LeConte so wisely urged on our attention.

Co-ordinate with this a like radical change has taken place in the rates of erosion, sedimentation, and the surficial aspects of the land. These climacteric stages in the extinction of species, in the introduction of species, and in surficial evolution furnish stimulating grounds for revision of ideas and a reconstruction of working tenets. They offer a point of departure of most suggestive value.

A very vital phase of this recent physico-dynamico-bio-psychic combination is human art and industry. This becomes a very important additional resource in discrimination. The evolution of the arts and industries is usually far more rapid and much more sensitive to conditions than the evolution of biotic species. It is in like degree a more refined criterion of historical progress.

(3) The third line of approach is an endeavor to find in the record of work done by the chief terrestrial agencies a basis for the natural events that mark off the progress of the ages. These agencies are more or less rhythmical in action and so reach climaxes here and there that serve for dividing lines or points of reference. To a large degree these agencies are the basal causes of the leading events of the geologic record. As such basal causes they are eminently fit for true criteria. Concretely, the physico-dynamic agencies record themselves in changed relations of land and sea, in changes of surface attitude, in changes of soil growth, of denudation, of configuration, and of sedimentation, as also in changes of climate, and of most other environing features that condition life. We have already remarked that glacial invasions and glacial oscillations have already in large measure replaced the older criteria on which are based the divisions of the Pleistocene Period and its separation from the Recent Period. A greatly enlarged use of these newer physico-dynamic criteria is scarcely less than imperative to conclusions that will stand the tests of the future.

(4) The help derivable from the spacial or positional relations of the deposits is most conveniently summed up in the term regional relations, and is best illustrated concretely. Between the Vero location on the very edge of the continent—far to the south—and the glacial formations in middle latitudes, there is a wide stretch of territory covered by Pleistocene and Recent formations, each of which has its place and significance in the recent history of the whole region. Whether we are able to distinguish all these or not, they are there and have their story to tell. These must ultimately be assigned their true place in the natural series that intimately connects the Vero deposits with the glacial formations in the northern states. These fall into four geographic provinces suited for work on the problem in hand. In the working of these provinces, of course, the extinctional, the introductory, and the physico-dynamical methods of inquiry should each play its own appropriate part. The following regional work is definitely indicated:

(*a*) Intensive and correlative work on the marine coastal tracts of the Atlantic and the Gulf. By this connection would be made between the glacial and glacio-natant deposits of the North and the contemporaneous marine deposits of the warm South. In this work the distinctive marine criteria, physical and biotic alike, should be adhered to rigorously and all formations not amply accredited by marine credentials should be left in the waiting list, or placed elsewhere.

(*b*) Inland from this narrow coastal tract, a broad U-shaped tract wraps about the Appalachians with its outer southeastern flank against the sea deposits of the Atlantic and Gulf on the one side and its inner northwestern flank on the glacial deposits of the Ohio Valley on the other, while at the top of the U these tracts abut against the glaciated areas on either side of the mountains. The strategic advantages of working this province as a unit are obvious. The aggradation-degradation deposits on the two flanks of the Appalachians offer special advantages in correlation and in interpretation.

(*c*) The fluvial deposits of the Mississippi Valley, embracing those whose sources lay in the glacial-drainage area of the upper

Mississippi Valley, offer a highly promising line of inquiry in which definite glacial-drainage stages may probably be traced into physical connection with both the terrestrial and the marine deposits of the more southerly regions.

(*d*) The lowland and upland plains west of the Mississippi offer a similarly promising connecting tract between the Keewatin glacial fields and the Gulf coast.

The combined import of careful co-ordinated studies on these promising connective tracts between the fields of warmth and the fields of cold should greatly help in revising and reconciling the discordant interpretations so notably brought out—so fortunately brought out, shall we say,—by the singularly suggestive deposits at Vero.